lections have been purchased. One from the Jesup Fund, is a series of rare objects from the Tsimshian Indians of the North Pacific coast collected by Lieutenant G. T. Emmons. This fills practically the only gap in our series from that important culture area. The second collection, made by Dr. Carl Lumholtz, in the little-known borderland along the Mexican boundary of Arizona, was purchased from the Primitive Peoples of the Southwest Fund. Among the unusual pieces in this collection are the costumes of a fool dancer, consisting of a mask, a crude and useless bow and other absurd trappings. This is of especial interest since this ceremonial character seems to connect the Papago culture with that of the Plains. Among other things may be mentioned a series of wooden plows introduced into Mexico from Europe by the early Spanish explorers. The Papago are the southern representatives of the Pima stock and were found still practising the art of basketry for which the Pima proper were at one time famous. The collection contains excellent samples of this almost extinct textile art. The third acquisition, gained through the Jesup Fund, is the General U. S. Hollister collection of Navajo blankets. In this series there are sixty-six pieces, some made before 1850. In materials and dyes there is a full representation: eleven blankets of bayeta, one of natural wool, eight of native dyes, seven of Germantown yarn, twelve of other commercial yarn, and eighteen in aniline dyes. The four varieties of weave practised by the Navajo are fully represented. There are also a few exceptional blankets, one of which represents in its design the Corn God copied from the sand paintings of altars of the Navajo. This collection, jointly with the series recently presented by Mrs. Sage and those belonging to the Lenders and Tefft collections recently presented by Mr. Morgan, give us a series of Navajo textiles fully representative both as to technique and design.

UNIVERSITY AND EDUCATIONAL NEWS

UNIVERSITY COLLEGE, Reading, England, has received an endowment fund of $1,000,000. The donors are Lady Wantage $250,000, Mr. George William Palmer and Mrs. Palmer $500,000, and Mr. Alfred Palmer, $250,000.

IT is announced in European journals that a new Russian university has been founded in Rostov on Don. The medical course will begin the coming fall. In Jellaterinburk will be established an academy of mines, and in Voronez and Samara academies of agriculture.

WE are requested to announce that a vacancy has recently occurred in the position of assistant professor of zoology in the College of Medicine and Surgery in the University of the Philippines. The entrance salary is $2,000 a year, but if a man of exceptional ability is secured as much as $2,500 might be given. It is expected that the holder of this position will engage in research work, and there are zoological problems of great interest that can be investigated in the Philippine Islands.

PROFESSOR LAENAS GIFFORD WELD has resigned his position as head of the department of mathematics in the State University of Iowa. He resigned the deanship of the College of Liberal Arts two years ago, soon after the accession of the present State Board of Education.

DR. ADOLPH I. RINGER, of the department of medicine of Cornell University, has been appointed instructor in physiological chemistry in the University of Pennsylvania.

AT Ohio State University John H. Schaffner, associate professor of botany, has been advanced to the position of professor of botany and head of the department.

DISCUSSION AND CORRESPONDENCE

THE IMPORT OF VITALISM

PROFESSOR JENNINGS'S communication concerning "Vitalism and Experimental Investigation,"[1] like everything that he writes, does much to clarify the subject of which it treats. Yet I can not but think that some corners of the question still remain in a rather beclouded condition. It is apparent, at all events, that

[1] SCIENCE, June 16, 1911.

a previous letter of mine, upon which Professor Jennings is kind enough to comment, left room for certain misapprehensions. I venture, therefore, to ask for space both for the correction of those misapprehensions and for an attempt to carry the process of clarification a degree or two farther.

1. Professor Jennings is, of course, concerned with a question upon which I have touched only incidentally. I endeavored to discriminate and definitely formulate several doctrines which apparently tend to lose their identity under the common name of "vitalism"; Jennings points out that only one of the varieties of vitalism has any practical bearing upon experimental procedure. He seems, however, to suggest incidentally that this one is perhaps "the only kind worth distinguishing." Now, I should have supposed that any two or more things are worth distinguishing when they are in fact distinct and yet are likely to be confused. And that, in the use of the term "vitalism" and of its common antithesis "mechanism," a good deal of confusion has arisen seems to be beyond dispute. Most of the words ending in -ism, the current names of doctrines, need constant redefinition, or rather, constant care that they get and stay defined. "A French statesman," wrote Lord Morley recently, "some years ago told a public audience that if a patient linguist would only give them a rational dictionary of party appellations, such a one would earn a statue of fine gold." Men of science lack the facilities of French politicians for decreeing statues; but an exact and illuminating dictionary of party appellations is, if anything, even a greater desideratum in the domain of scientific and philosophical theories. Of the existing uncertainty about the meaning of "vitalism" and "mechanism"—and especially about the question whether the two terms are really to be taken as reciprocally exclusive, and as jointly exhaustive of the possible types of theory about organic processes—many examples might be given; I must be content with a few of especial interest in the present connection. Dr. E. G. Spaulding has, in a very interesting article, summarized the view of the late Professor Brooks upon the problem raised by Huxley's famous essay on "The Physical Basis of Life," in these words:

> Huxley's statement [that the properties of the protoplasm result from the nature and disposition of its molecules] can be granted to be valid, but . . . it does not mean that there is or ever can be an *a priori* deduction of the properties of protoplasm from those of its constituents; but that the connection between these must be bridged by induction. For the properties of the protoplasm, or, indeed, of the organism at any level, are not the additive result of those of the parts, but contain something quite new.

Now, with this position of his honored predecessor Professor Jennings seems to agree; in his illuminating address at Clark University[2] he said:

> As matter takes on new arrangements, its activities and reactions become different even though the properties of each constituent remain the same. . . . New methods of action arise when oxygen and hydrogen combine, producing water; new methods of action arise when a mass of brass and iron is arranged in the form of a clock. How, then, can it fail to be true in the case of organisms? . . . Hence we can not expect to find in the physics and chemistry of inorganic matter the full explanation of the properties of organisms.

The conceptions expressed seem to be identical. Now, Spaulding regards the position of Professor Brooks as "indicating the limitations of the mechanistic view of life"—though he adds that those limitations "are found as well in the inorganic realm."[3] Rádl (as Jennings has noted) expressly applies the name vitalism to this "idea that new methods of action arise when new combinations occur, taken in connection with the view that new combinations are found in living things." But Jennings regards this view as "far from a vitalistic one"; he calls it rather a "physico-chemical" or even "mechanistic standpoint."[4] Here, then, we find three expert writers on the subject giving two exactly contrary appellations to one and the same

[2] *American Journal of Psychology*, 1910, 349–370.

[3] *Popular Science Monthly*, February, 1911.

[4] *Op. cit.*, p. 364.

opinion. This can hardly make for lucid and fruitful discussion.

The prevalent confusion is illustrated once more in the "test of vitalism" proposed by another correspondent of SCIENCE.[5] We are asked to suppose an organism "instantaneously resolved into its constituent particles," and then put together again out of the same particles, each being impressed "with motions the same in direction and amount which they possessed at the instant of dissolution." Then, "if the reassembled body goes on as an organism as before, it will be proof that life is but the operation of . . . the ordinary mechanical and chemical forces." It surely would prove nothing of the sort. The possibility of the artificial production of life by chemical synthesis would logically be perfectly consistent with any of the several kinds of vitalism (including the doctrine of *Lebensautonomie,* which is what the writer quoted seems really to mean by vitalism), even with Driesch's notion of entelechy. There is in the nature of the imaginary experiment nothing indicated which excludes the hypothesis that an entelechy was originally in charge of the organism in question, that it, so to say, hovered hopefully about the scene during the process of decomposition, and promptly took charge of the proceedings again as soon as the original complex was recomposed. The proposed test, moreover, according to the proponent of it, permits "no sharp line of distinction" between one class of vitalists and the non-vitalists. It is not a wholly helpful way of defining non-vitalism to make it mean "one kind of vitalism."

2. The desirability of some effort to define and discriminate the "first" sense of vitalism —the doctrine of organic autonomy—is indicated by the fact that, both in the address cited and in his recent discussion, Professor Jennings seems to fail to distinguish that doctrine from something quite different and much less significant. This is shown in the passage already quoted about the "newness" of the methods of action characteristic, *e. g.,* of clocks; here the sense in which the uniqueness of vital phenomena is asserted by many vitalists is not differentiated from the sense in which every phenomenon under heaven may be called unique. The same misapprehension is shown in the concluding sentence of his letter, in which Professor Jennings (evidently referring to my "first" sense of vitalism) speaks of "the (for the working investigator) relatively inconsequential question as to whether anything happens in living things that doesn't happen in those not alive." This is undeniably a redundant question to raise, not only for working investigators, but even for otiose philosophers; but it is not a question which any one, so far as I know, has ever before proposed to raise. It certainly is not synonymous with the real question concerning the present possibility or intrinsic conceivability of the explanation of organic phenomena by the laws which describe the motion of inorganic particles—*i. e.,* of portions of matter *whether in or out* of those complexes called living bodies—which is the question over which "vitalists" and "mechanists" have been wont to debate. Clock-phenomena (to use Professor Jennings's illustration), however "new," are *not* autonomous with respect to the laws of physics; on the contrary, if you know the laws of physics (as a study of other inorganic bodies than clocks might reveal them to you) and know also the number, size, arrangement and composition of the pieces in a given clock (with due allowance for external forces), you can predict pretty well how the clock will behave. What the partisans of the doctrine of organic autonomy deny is that you conceivably ever can, from a study of the laws of motion of inorganic particles, arrive at a law from which you can predict how any living body will behave, *even if you know the number, size, arrangement and composition of the particles composing that body.* This question, about the ultimate relation of the laws of biology to those of the sciences of the inorganic, may not be susceptible of a demonstrative answer; but it is at any rate quite distinct from the banalities to which Professor Jennings refers. The question is one to which every reflective

[5] SCIENCE, June 2, 1911, p. 852.

mind would like to have an answer, if possible; it is one which many contemporary biologists (though chiefly through vaguely confusing it with other questions) suppose they have conclusively answered, in the one way or the other; and it is one upon which light may conceivably be thrown by the progress of experimental inquiry duly conjoined with logical analysis.

3. There is, as I have previously pointed out, another theory going under the name of vitalism which asserts organic autonomy, but also something more. It is the doctrine that certain vital phenomena are not dependent upon "any fixed configuration of material parts existing in the organism or cell at the moments at which the phenomena take place" —*i. e.*, that the same phenomena occur in a given organism in spite of profound modifications of the composition and configuration of the parts, through a sort of redivision of labor and redistribution of functions among the parts that remain. This doctrine is the substance of the conclusion which is suggested by Driesch's analysis of what is implied by the totipotency of parts in certain cases of morphogenesis, and by regeneration-processes.[6] This view seems to Professor Jennings tantamount to biological indeterminism and to a denial (so far as it reaches) of the principle of uniform causality. It is equivalent to an "admission that the principle on which experimental investigation is based breaks down when applied to biology."

A closer scrutiny of the doctrine's implications will, I think, disclose in it no such anarchical propensities. All that it logically need imply may be stated as follows: Within certain limits, at least some organisms are capable of realizing or maintaining the typical form of their species in spite of profound externally caused quantitative or qualitative changes in their physico-chemical mechanism; so that the "prospective potency" (at a given moment) of any single component particle is not a function of its own chemical nature *plus* the number and chemical nature of the other particles, but can be predicted only by means of a knowledge of the typical form of the species. But that typical form itself is a constant function of an original chemical compound of a specific type, viz., the fertilized egg of the given species. Hence, given the egg (or in the case of regeneration, the adult form) of a determinate species, everything about the process occurs in a regular, law-observing and experimentally investigable manner; only, *one* of the laws to be borne in mind is the law that the typical form of the species gets itself realized *despite* the radical mutilation of the mechanism and *by means of* a radical internal readjustment of the mechanism. There need in this be nothing arbitrary, nothing to baffle the purposes of the experimenter. It is open to him to ascertain by his usual methods how far, in a given organism, the morphogenetic units are "equipotential"; what are the limiting conditions of the organism's ability to maintain its typical form by the use of diverse internal mechanisms; and what are the steps of physical and chemical change by which the redistribution of functions and restoration of structure get accomplished. A "machine," for Driesch, is any system, each

[6] It was to this doctrine alone that I referred in the passage upon which Professor Jennings comments at p. 931, note 12; I was not, as I supposed the context made clear, attempting a summary of Driesch's whole system. But I appear to have expressed myself ambiguously, and am glad, therefore, to have Jennings call attention to the fact that Driesch's theory is "not limited to morphogenesis." It is, however, true that the most distinctive and novel thing in that biologist's doctrine is his conception of "harmonious equipotential systems"; as he himself declares, what is really characteristic in neo-vitalism is due "to the renaissance of experimental morphological inquiry, to the 'Entwicklungsmechanik' of Wilhelm Roux; all the new factual evidence for the doctrine of *Lebensautonomie* has been found in this field" (*Der Vitalismus*, 1905, p. 155). For a discussion of the import of Driesch's arguments from behavior, and their relation to his arguments from morphogenesis, space is lacking here. Though I think Jennings misconceives Driesch's position in ascribing to him a wholesale "experimental indeterminism," I do not wish to complicate the discussion with exegetical inquiries into the precise meaning of a rather difficult writer.

of whose parts performs its specific function, in relation to the action of the whole, only by virtue of its composition and its spatial relations to the other parts; thus, if those relations are sensibly altered, the whole will no longer function in its original manner. If this were true of organisms, their action would, in Driesch's sense, be "mechanical," even though the law of the action of the parts were not deducible from any law of inorganic mechanics.[7] But since some or all organisms are, at least to some extent, harmonious equipotential systems, their action is not mechanical in either sense—such is the essence of the argument. "It must be granted that a machine, as we understand the word, might very well be the motive force of organogenesis in general, if only normal, that is to say, only undisturbed development took place, and a taking away of parts of our system led to fragmental development."[8]

In all this argument for the non-mechanical nature of organic phenomena there is nothing whatever that necessarily "exempts from experimental determinism . . . that immense field of developmental processes which lies between the egg and the adult," or that necessarily nullifies the experimentalist's postulate that "when two cases differ in any respect there will always be found a preceding difference to which the present difference is (experimentally) due." The argument (whatever its worth) does not imply that different effects have the same antecedents; it implies only that, in an individual organism, the *same* type of effect (namely, the typical form of the species) may follow from *different* antecedents —the relation between the two sets of antecedents being such as to reveal the non-mechanical character of the action of both. It is surprising that this, of itself, should be regarded as violating the rule of causal uniformity, since that rule notoriously does not work both ways; the same effect (in the ordinary sense) need not always have the same cause. Even if entelechies are to be dragged into the situation, indeterminism need not follow, if only it be assumed (what nothing in the hypothesis precludes) that an entelechy always comes into action whenever a specific material complex has been formed; and that the occasions upon which, and the manner in which, the entelechy determines the subsequent action of that complex are uniform.[9] I do not say that Driesch himself clearly and consistently adheres to this assumption; but in so far as he departs from it, and gives color to the charge of indeterminism, he introduces a foreign element into his conception of a "harmonious equipotential system," and confounds the second sort of vitalism with yet a third essentially distinct one. And this is one of the confusions which it is needful to guard against in the discussion.

4. Let me briefly revert in conclusion to the original question concerning the meaning to be assigned to the term vitalism. Professor Jennings would apparently reserve that word for indeterminist theories, on the ground that these alone are likely to have much interest—the interest of the repulsive—for "the man of science at work with his two hands." It does not seem quite clear that the limitations of interest of even bimanous experimentalists ought to be erected into a canon of lexicography; yet one should welcome any canon which will impose upon the terms used in the discussion of vitalism single and definite and constant meanings. It is of no importance whether a given trisyllable denote one or another doctrine; it is of some real importance that it be not used indiscriminately to disguise the real nature of several distinct doctrines, and that these doctrines themselves, the distinctions between them, and their bearings both theoretical and practical, be clearly formulated and understood. So far as the tendency of present technical usage is con-

[7] Driesch himself does not seem to note the distinction here indicated, and accordingly frequently speaks as if he were arguing merely for organic autonomy in the ordinary sense.

[8] "Science and Philosophy of the Organism," 1908, I., 139.

[9] This uniformity would not imply (as the hasty reader may incline to suppose) that entelechy-determined action and mechanical action would be the same.

cerned, I am not sure that the meaning preferred by Professor Jennings is the most widely accepted one. Eisler, for example, in the last edition of his "Wörterbuch der philosophischen Begriffe" defines "Neo-Vitalismus" primarily as the doctrine which "betont die *Autonomie* und *Aktivität* der Lebensprozesse, die Unmöglichkeit diese restlos aus mechanisch-chemischen Gesetzen abzuleiten"; and though he adds to this formula (which he ascribes in common to Bunge, Wolff, Reinke, Hartmann, v. Uexküll, K. C. Schneider and Driesch) some peculiarly Drieschian details, these do not amount to a theory of "biological indeterminism."

Usage, however, is still too various and confused to settle the matter; and none of us has authority to legislate upon the subject. The term vitalism might, with real advantage to both biology and philosophy, be retired from service. Even if that desirable consummation be past hoping for, it should still be possible to persuade contributors to the discussion to bear in mind the ambiguity of the term and of the antithetic "mechanism," and to recognize and keep separate the several distinct issues which in much current use of those terms tend to become blurred and confused.

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The Johns Hopkins University,
June 19, 1911

SUBSIDENCE OF ATLANTIC SHORELINE

On page 906, of Science, No. 858, I observe certain statements of D. W. Johnson, of Harvard, maintaining that there is no decisive evidence of recent subsidence of the Atlantic coast regions, but, on the contrary, some beach-formations which would seem to prohibit such conclusion. This is all very startling, not to say iconoclastic.

The great shallow bays of our more southern coasts, such as Delaware, Chesapeake, Albemarle and Pamlico, having long estuary-like arms, which suddenly and bluntly terminate at their upper ends and there receive in every instance a stream of comparatively small size, might at first seem to be a somewhat puzzling geographic condition; but it can readily be accounted for through subsidence and in my opinion in no other way.

At the maximum of the last elevation of the coast, the Susquehanna River flowed southward, with sensibly more than its present volume, and emptied into the Atlantic near the present Cape Henry. A few miles above this point it received from the west a moderate stream following the direction of the present James River. Higher up another moderate stream, following the line of the present Potomac, joined the Susquehanna near the present Smith Point.

For some thousands of years, perhaps, constant denudation lowered and flattened out the land along these streams. A subsidence of the coast then began. The sea, entering the Susquehanna, formed at first a small bay which received both the curtailed Susquehanna and the James. With still further subsidence the ocean filled more and more of the river valley and those of its branches, until, after a subsidence which need only amount to some 75 feet, we find the long shallow Chesapeake and its lateral arms formed by the intruding ocean as we know them to-day. The same reasoning applies to the other bays mentioned. Further north these results are less manifest because of the more precipitous nature of the coast; but the great terminal moraine constituting the backbone of Long Island became separated from the mainland by the waters of Long Island Sound, and it is probable that Narragansett Bay was largely formed in the same way.

If the nature of these shallow bays and their long, wide, abruptly ending lateral arms, receiving in every case at the upper end a flowing stream, is not positive evidence of progressive subsidence of the coast in recent times, it would be difficult to imagine any satisfactory reason for the observed facts. The evidence seems, in fact, as plain as though written in bold characters for us to read.

Other evidence of subsidence is shown by the salt marshes, with perfectly level surfaces built up by vegetable débris at high-tide